we are not surprised to find that Jellett's work is difficult of access to general readers, and on this ground, if on no other, we welcome the present attempt to bring the "Calculus" to the fore again. The author follows Airy and Todhunter in the view he takes of a variation, and Jellett and Strauch in the treatment of varying functions, but he has not neglected to give fairly full accounts of the conceptions and methods of other writers.

A good deal of the preface is taken up with details which might well be omitted should the work reach a second edition, as we hope it may.

There are in all five chapters (568 and xvii. pages) printed in good type and in excellent style.

Chapter i., maxima and minima of single integrals, involving one dependent variable is broken up into ten sections: Chapter ii., maxima and minima of single integrals involving two or more dependent variables in two sections: Chapter iii., maxima and minima of multiple integrals in six sections. Chapter iv., applications to determining the conditions which will render a function integral one or more times in two sections.

Chapter v. gives a historical sketch of the rise and progress of the calculus of variations founded upon Todhunter's "History," and closes with an account of the "Researches in the Calculus of Variations," referred to above.

We have nothing to say of Mr. Merriman's work in addition to what we have said already (NATURE, vol. xxx. p. 334): the works are identical, except in the title pages.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Italian Aid to Biological Research

THE Committee appointed by the Royal Academy of the Lincei in Rome at the request of H.E. the Minister of Naval Affairs, to see that the best possible use in the interests of science be made of the natural history specimens collected by officers of the Royal Italian Navy, wishes to make known to all students of biology that rich material for study, consisting of a certain number of plants and extensive collections of animals of nearly all classes, is at present deposited at the Zoological Station at Naples. This material has all been collected by the officers of the Royal Navy, principally by the *Vittor Pisani* in a recent voyage round the world, and by other Italian men-of-war in the Red Sea and the Ægean Sea. These collections have been preserved by the best and most modern methods, and can be used for histological and morphological researches, in accordance with the actual requirements of science, as well as for systematic and faunistic investigations. The Committee places this rich material at the disposal of the men of science of all countries who will ask to take part in its illustration, either to complete monographs in course, or for monographical works or for special research on any organic system of a given group.

The requests, on which the Committee will decide, are to be sent to Prof. Trinchese, University of Naples.

Prof. TRINCHESE, Naples
Prof. TODARO, Rome
Prof. PASSERINI, Parma
Prof. GIGLIOLI, Florence
Lieutenant CHIERCHIA (Royal Italian Navy), Naples
Prof. DOHRN, Naples

The Resting Position of Oysters

IN a letter from Mr. J. T. Cunningham in your impression of NATURE of October 22 (p. 597) it is sought to show that the oyster does not rest on its left but on its right valve. The evidence which appears to him conclusive on this question is "that the right flat valve is always quite clean, while the convex valve is covered with worm-tubes (*Stylea grossularia*) and Hydroids."

This observation is correct on the whole, but not decisive for the question under consideration. After reading Mr. Cunningham's letter I proceeded to examine 140 oysters in my collection of Schleswig oysters, and found only on a few right valves a worm (*Pomatoceros tricuspis*) or a Cirripede (*Balanus crenatus*), whereas on many *left* valves I distinguished sponges (*Halichondria panicea*), *Alcyonium digitatum*, Hydroids (*Sertularia argentea*, *Tubularia indivisa*, *Eadendrium rameum*), Bryozoa (*Alcyonidium gelatinosum*), *Balanus crenatus*, *Pomatoceros tricuspis*, or *Sabellaria anglica*. Of the 140 oysters examined 43 still bore on their shells the body on which as spawn they had reared themselves, namely pieces of oyster-shells, *Mytilus edulis*, *Mya arenaria*, *Mya truncata*, *Cardium edule*, or *Buccinum undatum*. All these adherent bodies were attached to the nucleus of the left valve, not one single piece to the nucleus of the right. And this is a circumstance decisive in the question raised by Mr. Cunningham. The places on the right valve, where living animals rest, did not stick close to fixed bodies, but the water flowed freely over them, thus admitting embryos to settle on them. The bottom of oyster banks is not a smooth surface, but is formed mainly of old oyster-shells on which many living oysters do not assuredly plant themselves closely and horizontally, but lie often obliquely. It is thus that Hydroids, Sponges, Alcyonium, and Alcyonidium, having settled on the right lower valve, are enabled to grow freely in the water and without let or hindrance develop to the length of four or five inches.

Kiel, October 31 KARL MÖBIUS

Universal Secular Weather Periods

I DO not want to pose as a statistical cycle-hunter, or to bolster up any mere apparent local periodicity of a certain meteorological element, but I wish to place before your readers the appended independent paragraphs from two journals, one on each side of the Atlantic, and to ask any unprejudiced person if we have not here some preliminary evidence (all the more valuable from its being so evidently incidental and unconscious) in favour of the march of certain secular weather areas, possibly connected with barometric waves, similar to those traced out by Messrs. Chambers and Pearson in India, across the Atlantic, from America to Europe.

I would not submit such slender evidence to criticism were it not that it concurs entirely with certain views put forward by myself in a recent paper in the Royal Meteorological Society's *Quarterly Journal*, on "The Height of the Neutral Plane of Pressure in India," and that I have long felt that the entire question demands attention both on scientific and economical grounds. Also both paragraphs include last year, thus bringing the apparent periodicity up to date.

Being at present fully engaged in two other branches of research, I am unable just now to take up this hopeful and important problem, but I would suggest that if we ever intend to forecast the *general character* of the weather of a season or year, which even in this country undergoes long periodic changes, during which it remains for weeks and months together of the same *type*, some such method as the following must be adopted :—

Annual and seasonal mean barometric charts must be constructed from records at principal stations in America, Europe, and Asia for the past fifty or sixty years, and from them barometric abnormals for each year, and for each season, must be calculated, and charted. An examination of these ought to throw great light on, if not to some extent solve, the question of the motion of the larger pressure areas which in turn guide and control the motion of the smaller diurnal systems. The work would, I admit, be one of some considerable magnitude, but surely it is one imperatively demanded in the interests of the science, besides being *a priori* likely to yield valuable results. It has long been a cherished idea of mine to endeavour to carry out the scheme myself, and it is only because I feel precluded from doing so at present by the pressure of other work that I throw out the suggestion for the benefit of any who feel disposed to take it up.

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The paragraphs are as follows:—

Mr. Baldwin Latham, in a discussion which ensued upon the Report of the Committee on Decrease of Water Supply (*Quarterly Journal* Roy. Met. Soc., p. 223), said:—

"The records showed that there appeared to be a recurrence of low water every ten years. There was lower water in 1824 and in 1835; the period 1844–5 was low, especially when compared with the years immediately before and following; 1854 was remarkably low; also 1864–5, 1874–5, and now they come to the present low period of 1884–5.

"As to what was the cause of this marked periodicity it was very desirable to ascertain, and, having pointed it out, probably some light might be thrown on the subject."

The other is from the *American Meteorological Journal* under the heading "Cold Winters in Michigan," and the writer says:—

"It is interesting in this connection to notice that the local reports of extremely cold winters place them at intervals of between ten and eleven years. . . . The winter of 1842–3 is thus shown to have been extremely cold; also the winter of 1853–4; the winter of 1863–4 noted for its terribly cold new year; the winter of 1874–5, when the frost penetrated into the ground in Port Huron four to six feet, there being scarcely a thaw between January 1 and the middle of March; and, lastly, the winter of 1884–5, which beats the record for extreme cold during January and February."

I may add that before I had seen either of these paragraphs I had concluded from other sources that the years 1821–2–3–4, 1833–4, 1844–5, 1866–7, and 1875–7 were characterised by mild winters in Europe and unusual cold in Iceland and America, being preceded in most cases by drought during the summers; but of course this represents merely the result of a preliminary glance at some general records of noteworthy seasons.

November 9 E. Douglas Archibald

Photography of the Corona

I HAVE been following with interest the communications which have been made from time to time to *Science* by Mr. W. H. Pickering regarding the photography of the corona in full sunshine. Whilst admiring the manner in which he has built up his theoretical objections to its possibility, I am forced to dissent from his deduction from the fact that the theory does not fit in with the results actually obtained during the eclipses observed in Egypt and the Caroline Islands. I have in my hands at present spectrum and other photographs of the corona made during the expeditions to those localities, and from them I gather he has evidently much underestimated the photographic brightness of the corona as compared with that of the sky. As I propose shortly to read a paper before the Royal Society on the subject, I cannot enter into details at the present moment. All I will say is that the comparative photographic intensity of both can be estimated with approximate exactness from the data I have by me.

I write this for insertion in your columns, as in your last issue you have a note regarding Mr. W. H. Pickering's communications on this subject. W. de W. Abney

Permanence of Continents and Oceans

MANY naturalists are accustomed, in lecturing, to speak of the existing ocean basins as "permanent." Though this must to a large extent be a true statement, many geologists at all events must be perfectly aware that the former distribution of life requires that nearly all lands, however remote at present, must have been, perhaps more than once, in connection with each other. Tropical South America is perhaps the most isolated continental province now existing. I would ask these naturalists to explain how its species of tropical genera not peculiar to it got there, and how many of them came to be represented in Europe in Tertiary times.

That the lands are always chiefly centred about the same spots, and also the converse, would, I think, be an acceptable way of putting it; but that the Atlantic was never bridged except towards the Arctic and Antarctic circles, is a statement that is unwarrantable because contradicted by unimpeachable evidence. J. Starkie Gardner

History of Elasticity

IN order to estimate Poncelet's services to the theory of elasticity I am extremely desirous of examining certain works by him. These works are not to be found in the London or Cambridge Libraries, and the Paris booksellers to whom I have applied despair of being able to procure copies. It will hardly be possible for me to go to Metz to examine them before the publication of the first volume of the "History of the Mathematical Theories of Elasticity." Possibly some of your readers may know of the existence of accessible copies in this country. If so, I should esteem it a great favour if they would communicate with me at University College.

In 1827–29 Poncelet gave at Metz a "Cours de Mécanique Industrielle aux Artistes et Ouvriers Messins." In this "Cours" various important points of theoretical elasticity were considered for the first time.

It was published as follows:—

(*a*) Part I. Lithographed edition, Metz, 1827.

(*b*) Part II. First edition lithographed 1828; second edition lithographed 1831.

(*c*) Part III. Lithographed edition, 1831.

(*d*) Part I. First printed edition, Metz, 1829; second printed edition, Metz, 1831.

It is needful to remind your readers that there are numerous other works entitled "Mécanique Industrielle," by Poncelet, published at Liége, Paris, and Brussels, differing from each other, and entirely from the above. These I have examined, but they do not contain what I require. Karl Pearson

University College, London, November 15

The Heights of Clouds

In the very favourable notice of our "Mesures des Hauteurs et des Mouvements des Nuages," in NATURE of October 29 (p. 630), there exists a misunderstanding as to the probable errors of our measurements, which makes our observations seem much more inexact than they really are. I therefore ask your permission to correct it.

Mr. W. de W. A. says: "Perhaps one of the most easily-observed clouds is the cumulus, and we find from a table given that the *probable error of observation* is very considerable." But, in fact, what is there referred to as an error of observation is not such an error; it is the probable uncertainty ("incertitude probable") *depending on the variability of the phenomenon itself.*[1] This is expressly stated in the treatise. On p. 39 (that following the table quoted) there may be read: "L'incertitude calculée comprend ainsi et celle dépendant de la variation des hauteurs des nuages, et celle provenant des erreurs d'observation. Celle-ci est cependant assez petite par rapport à la première et à peu près constante pour les différentes heures du jour, comme on le trouvera en la calculant séparément à l'aide des erreurs moyennes *m*." That mean error *m* is just the *mean error of observation* in the height of a cloud, and in our "list of observations" we have given it for every observation, as well as the corresponding mean angular error of the observation. By calculating the probable error of an observation of cumulus by means of those values of mean errors we have found it to be 35 *metres* (instead of 748 metres, as Mr. W. de W. A. thinks it to be), and the probable error of the mean is found to be 3 metres (instead of 40 metres), the whole number of observations being 134.[2]

Thus the above assertion is fully justified, viz. that the errors of observation may be quite neglected when compared with the uncertainty depending upon the variability of the heights of the cumuli from one cloud to another. That variability is itself a phenomenon worthy of investigation, varying as it does according to the hour of the day and the barometrical state of the weather, and that is the reason for which we have calculated it. As to the mean angular error in observing a cloud, we have found it very often to be inferior to that obtained in observing the centre of the sun, and in all the better observations that error is fully comparable to the error in observing the sun, as may be seen from our treatise. This proves that, for such observations, the uncertainty of an identical point of cloud did not exist at all, the whole uncertainty depending on the unavoidable instrumental errors. Still it may be that the errors are

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[1] For the figures in the table quoted represent simply the *probable difference* of an observation (of the mean found) from the *true* mean calculated by the method of least squares.

[2] For the higher clouds, as the pure cirri, this error was often very great indeed, but it was so because their distance was much too great when compared with our basis, the parallax obtained being not greater than 1° or 2°. This year (1885) the measurements are regularly pursued from the ends of a basis of 1302 metres, and we can now determine with great accuracy the height even of the most elevated cirri, as well as their horizontal and vertical velocities.

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